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ORIGINAL PAPER

The Impact of Imprisonment on Marriage and Divorce: A Risk Set Matching Approach

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Abstract Marriage has a prominent place in criminological theory and research as one institution that has the potential to genuinely foster desistance from a criminal career. Mass imprisonment policies in the United States and elsewhere, therefore, pose a potential threat of increased crime if they impede the ability of ex-prisoners to reintegrate into society by stigmatizing them and limiting their chances in the marriage market. We use a long-term study of a conviction cohort in The Netherlands to ascertain the effect that first-time imprisonment has on the likelihood of marriage and divorce. The results suggest that the effect of imprisonment on the likelihood of marriage (among unmarried offenders) is largely a selection artifact, although there is very weak evidence for a short-lived impact that does not persist past the first year post-release. This is interpreted as a residual incapacitation effect. On the other hand, the results strongly suggest that the experience of incarceration leads to a substantially higher divorce risk among offenders who are married when they enter prison.

Keywords Incarceration · Marriage and divorce · Propensity score · Risk set matching

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Introduction

Although the rate of incarceration has slowed in the United States, at yearend 2006 the incarceration rate (inmates in state or federal prisons, or in local jails) was an astonishing 751 per 100,000 residents, with a total confined population of almost 2.3 million (Sabol et al. 2007, p. 4). In Europe as well, two-thirds of the 35 countries surveyed experienced growth in their incarceration rate during the latter half of the 1990s and the early 2000s (Aebi et al. 2006), albeit with incarceration rates that are a fraction of the US except in rare instances (e.g., Russia). The Netherlands in particular, long known for its liberal penal policies and comparatively low incarceration rate, witnessed remarkable prison growth from the late 1970s to the present and currently has among the highest incarceration rates in western Europe, at approximately 160 per 100,000 in 2005 (Downes 2007; Downes and van Swaaningen 2007; Tonry and Bijleveld 2007).

A portion of the expansion in custodial sanctions—in the US and elsewhere—is attributable to the fact that sentence lengths have increased for imprisoned offenders. An additional influence, however, is growth in the use of incarceration as a presumptive sanction for offenders that would historically have been assigned a non-custodial sanction—a phenomenon known as "net widening." A growing international literature has been attentive to the collateral consequences of net-widening imprisonment policies, observing that the social costs of steadily rising prison admissions in an era of rapidly declining crime rates might very well outweigh the crime-control benefits (Hagan and Dinovitzer 1999). The potential irony of mass imprisonment is that, to the extent it has unintended adverse effects on life outcomes that are correlated with criminal offending, large-scale growth in the incarceration rate may actually exacerbate the crime problem over the long run by stigmatizing an ever larger class of individuals (Pettit and Western 2004; Uggen et al. 2006).

One unintended consequence faced by ex-inmates might be felt acutely in the institution of marriage. Specifically, a prison record might erode one's prospects for family formation by delaying or permanently disrupting the transition to marriage. Imprisonment might also disrupt intact marriages by increasing the risk of divorce. In light of compelling research demonstrating an inverse relationship between marriage and criminal behavior, such difficulties could inadvertently sustain an individual's criminal career.[1] There are potential side effects also for crime control at an aggregate level. Mass imprisonment might very well diminish the capacity of communities to exercise informal social control by further weakening already fragile families (Lynch and Sabol 2004; Rose and Clear 1998; Western et al. 2004).

In this study, we are interested in quantifying the causal impact that incarceration has on an individual's likelihood of marriage and divorce. We employ data from a long-term study of a conviction cohort of Dutch offenders. Our study is unique in a number of important respects. For example, we use panel data to prospectively identify periods of imprisonment and assess the short- and long-term impact of such experiences on marriage formation and stability. Because of the real possibility of endogeneity in the incarceration–marriage relationship, we use as a comparison (i.e., counterfactual) sample only those

[1] For example, Blokland and Nieuwbeerta (2005); Farrington and West (1995); Horney et al. (1995); King et al. (2007); Laub et al. (1998); Laub and Sampson (2003); Ouimet and LeBlanc (1996); Piquero et al. (2002); Sampson and Laub (1990, 1993); Sampson et al. (2006); Shover (1996); Warr (1998). These studies rely heavily on samples that are considerably high risk, for example, Piquero et al.'s (2002) prospective analysis from a sample of serious male offenders released from the California Youth Authority. For a much more qualified perspective on the salience of marriage, see Giordano et al. (2002, 2007).

individuals who were convicted of a crime at the same age but were not sentenced to a custodial sanction, and use propensity score estimation to select from these a subset of individuals who were at high risk of imprisonment.

The remainder of this paper is organized as follows. We review research on the effect of incarceration on marriage, and proffer several theoretical mechanisms that might account for a disruptive effect of incarceration on marriage formation and stability. We then describe the scope of the present effort to estimate the incarceration–marriage relationship. Finally, we present the empirical results followed by an extended discussion and concluding comments.

Existing Research on Incarceration and Marriage

Three datasets provide the only quantitative evidence pertaining to the effect of imprisonment on the likelihood of marriage. For example, data from the Glueck delinquents support the notion that juvenile incarceration disrupts marital unions in adulthood. Sampson and Laub (1993) found that men sentenced to a reformatory as juveniles were more likely to be divorced in young adulthood (ages 17–25) and middle adulthood (ages 25–32). In their model of the propensity to marry (in order to estimate the effect of marriage on crime), Sampson et al. (2006) found that the length of juvenile incarceration was inversely correlated with the probability of marriage throughout adulthood and into the seventh decade of life. Thus, early imprisonment appears to erode a man's long-term marriage prospects. Evidence from the Glueck men also suggests that incarceration impedes marriage formation in adulthood, both contemporaneously and in the short term. For example, Sampson et al. (2006) found that the length of incarceration in the previous year (as well as cumulative incarceration length) was inversely correlated with the probability of being married in the current year.

A pair of studies utilized the Fragile Families and Child Wellbeing Study (FFCWS) to estimate the effect of incarceration on family formation among a high-risk sample of parents of newborn children. Western and McLanahan (2000) found that the cumulative incarceration status of the father (at the first follow-up) was strongly related to whether or not the family was intact, that is, whether the father resided with the child's mother in either a cohabiting or marital situation. Specifically, an incarceration history (as reported by the father) reduced the odds of living together by 49% (odds ratio = 0.51). Using the same dataset, Western et al. (2004) reported that the father's incarceration history (as reported by either the father or mother) was inversely associated with the likelihood of cohabitation and marriage relative to non-residence (see also Western 2006).

Several recent studies using the National Longitudinal Survey of Youth 1979 (NLSY79) have investigated the relationship between incarceration and marriage. Western (2006) observed that 40% of males with a prison record remained unmarried until age 40 compared to just over 10% of never-incarcerated males. Lopoo and Western (2005) reported that male offenders were significantly and substantially less likely to get married during periods in which they were interviewed in an institution. In their discrete-time event history model of first marriage using the 1979–2000 interviews, they estimated that the odds of the transition to marriage were 78% lower (odds ratio = 0.22) for incarcerated individuals compared to their non-incarcerated counterparts. Similar results were reported from the 1983–2000 interviews by Huebner (2005; for race-specific analyses, see Huebner 2007) in a fixed-effects model of marriage, in which she estimated that the odds of being married were 39% lower for currently incarcerated individuals (odds ratio = 0.61). One notable exception was the study by Raphael (2007) using the 1979–1996 interviews, who found that current incarceration had no impact on the probability of never being married once individual effects were controlled.

With respect to the long-term (versus contemporaneous) relationship between incarceration and marriage in the NLSY79, Huebner (2005, 2007) reported that a history of incarceration as an adult—from the 1983 survey onward, when all respondents were at least 18 years of age—was inversely associated with the probability of marriage, but that youthful incarceration (prior to the 1983 interview) was not correlated with marriage. Raphael (2007) noted that having ever been incarcerated prior to the current interview was associated with about a 14-point increase in the probability of having never been married. Lopoo and Western (2005), on the other hand, observed no such effect of having ever been incarcerated on transitions to marriage and divorce. The preponderance of evidence from the NLSY79 suggests, therefore, that incarceration is inversely correlated with marriage, although when measured contemporaneously, this is conceivably an incapacitation effect of prison on marriage likelihood. The findings about the long-term or accumulative effect of incarceration on marital transitions are more equivocal.

Research on the likelihood of divorce is rarer still, but the results are quite consistent. Lopoo and Western (2005) and Western (2006) found that a transition to divorce was significantly more likely to occur contemporaneous with periods of incarceration in the NLSY79. In particular, there was an astonishing twofold increase in the odds of divorce among individuals who were incarcerated relative to their non-incarcerated peers (odds ratio = 2.99). In the FFCWS, Western (2006) similarly found that incarceration significantly increased the risk of separation for men in marital or cohabitational unions, and in fact it was among the strongest predictors in his model (odds ratio = 2.23). Thus, incarceration appears to have a major disruptive effect on pre-existing unions.

Theoretical Considerations for the Impact of Incarceration on Marriage and Divorce

A number of plausible theoretical explanations can be marshalled to interpret the inverse correlation between incarceration and marriage among unmarried offenders, or the positive correlation between incarceration and divorce among married offenders. We consider several possibilities below, although we caution that the review is not intended to be exhaustive.

Signaling and the Stigma of Imprisonment

One mechanism by which incarceration would conceivably erode marriage prospects is through the stigma of imprisonment and the "signal" that such a label transmits to potential spouses. From the perspective of signaling theory and its sociological counterpart—labeling theory—a prison record conveys (however, imperfectly) information about a person that entails more than just his or her future risk of criminal behavior. For example, it might communicate something about one's prospects for success in the labor market, about one's ability to provide for a family, about the company that one chooses to keep, about a potential spouse's risk of becoming a victim of domestic violence, and so on. In other words, a label of "ex-inmate" provides important information that is used by prospective spouses to assess the labeled individual's reputation, integrity, and future prospects—criminal and non-criminal alike. If potential spouses are responsive to cues about one's "marriage potential," it is conceivable that a prison record conveys, quite simply, that one is not marriage material. Incarceration would thus lead to exclusion from the pool of eligible marriage partners because one is viewed as a less viable and attractive partner (see Wilson 1987; Wilson and Neckerman 1986).

This perspective has been prominently featured in recent empirical work studying the effect of incarceration on employment prospects, demonstrating that incarceration has a detrimental and non-trivial impact on one's employment prospects by reducing wages and earnings (e.g., Western 2002). In a Milwaukee study of matched audit pairs, Pager (2003) found that employers advertising entry-level job openings were less than half as likely to call back applicants who reported a criminal history (a felony cocaine trafficking conviction with 18 months prison time). The unambiguous conclusion was that "criminal records close doors in employment situations" (p. 956). A prison record would represent a stigma in the marriage market as well if potential spouses impose a penalty for the economic deficits that tend to accrue to ex-inmates.

In light of existing knowledge about the generality of offending, moreover, offenders with a criminal history might be more likely—or more importantly from a signaling perspective, might be *perceived* as being more likely—to commit violence against their intimate partners. For example, Carbone-Lopez and Kruttschnitt (2007) discovered that female inmates were at qualitatively higher risk of intimate violence prior to their imprisonment when they were involved with men who themselves had a history of criminal activity. Western (2006) noted that fathers with a prison record were significantly and substantially more likely to inflict physical injury on the mothers of their children within 12 months of the child's birth. Consequently, individuals with a prison record might signal to prospective (or current) spouses that they are potentially violent.

Social Networks and Restriction of Marriage Opportunities

The foregoing stigmatization perspective presumes that current or prospective spouses perceive ex-inmates to be undesirable marriage partners. However, stigmatization might operate in a second, related way, by constraining the social networks of ex-inmates and thereby limiting the opportunity to meet potential dating or marriage partners. While crime-prone individuals might run in social circles and frequent geographic locations dominated by like-minded others (through a process of self-selection or social homophily), incarceration and the formal labeling process might exacerbate this tendency through a process of subculture formation among similarly stigmatized individuals (see Braithwaite 1989). To the extent that policies of mass imprisonment create a society of outcasts through processes of social exclusion (Becker 1963; Garfinkel 1956; Link et al. 1989), incarceration might result in growing isolation from conventional peer contexts and growing integration into criminal peer networks. This "criminal embeddedness," or social embeddedness in crime, would provide fewer opportunities than before to meet marriageable partners (see Hagan 1993).

Incapacitation and "Time Out" from Marriage

For married offenders, incarceration imposes a period of time-out from the marriage. Because incarceration often removes offenders from their communities of residence, married inmates are physically isolated from their spouses and families, which can lead to disruption in the quality of existing marital ties (Lopoo and Western 2005). The stress of separation could lead to withdrawal and dissolution as alternative sources of emotional support are sought by the "surviving" spouse. Additionally, the loss of economic support by the imprisoned individual might motivate a spouse to separate or divorce in the interest of finding a new partner who can help stabilize the financial situation in the household, particularly if children are present (see Phillips et al. 2006).

Criminal Propensity and Unobserved Heterogeneity

An entirely different view on the incarceration–marriage relationship flows from the possibility that individuals sort themselves into certain institutional settings on the basis of a differential tendency to consider the long-term consequences of their actions; what Gottfredson and Hirschi (1990) refer to as self-control. Because individuals with low self-control seek immediate gratification of their desires with minimal effort or long-term planning, they are less likely to be married or, if married, will tend to have difficulty maintaining a stable union: "People who lack self-control tend to dislike settings that require discipline, supervision, or other constraints on their behavior" (Gottfredson and Hirschi 1990, p. 157). Additionally, individuals with low self-control are theorized to be self-centered, insensitive, and non-verbal; qualities that would also likely interfere with union formation and stability. Simply put, non-marriage, an unstable marriage, and crime are all manifestations of the versatility of individuals with low self-control.

Contributions of the Current Study

This paper aims to advance empirical research on the causal impact of imprisonment on marriage formation and dissolution in several ways. First, any effect of incarceration might stem from its stigmatizing potential, which is more likely a consequence of the first experience of incarceration than of a repeat experience. Thus, we focus our analysis on estimation of the effect of *first-time imprisonment*. This focus allows us to sidestep the problem of feedback effects whereby imprisonment affects (reduces) the likelihood of marriage, the lack of which in turn affects (increases) the likelihood of imprisonment. Such non-recursivity increases the risk that any estimate of the effect of imprisonment on marriage is contaminated by simultaneity bias; specifically, that any inverse relationship between imprisonment and marriage is overestimated. We recognize that a focus on first-time imprisonment limits the generality of our findings, but balanced against this cost is the benefit of elimination of one important (and plausible) source of endogeneity.

Second, perhaps the most serious threat to the validity of a finding that incarceration disrupts marriage stems from the observation that imprisonment is the sanction reserved for individuals who commit the most serious crimes and individuals with the lengthiest criminal histories. This is the problem of "stochastic selectivity" (Blumstein et al. 1993). Great care must be given to accounting for a selection process that will tend to make imprisoned individuals disproportionately prone to non-marriage or unstable marriage compared to non-imprisoned individuals, simply because of the way that the criminal justice system, at each decision point (arrest → charging → prosecution → conviction → incarceration), filters only the highest-risk offenders in the pool through for further processing. What this means in practice is that the most valid comparison sample for imprisoned offenders is, other things equal, a sample of offenders who have been filtered just as far into the criminal justice system but short of incarceration. We thus limit our attention to individuals who have at least been convicted of a crime and employ *propensity score matching* specifically designed to balance a variety of time-stable and time-varying indicators of incarceration risk. Specifically, we employ a matching approach known as "risk-set matching" that conditions directly on age of first incarceration. The estimated propensity score in a risk set matching model represents a hazard, or the probability of imprisonment among those with no prior prison experience.

Third, we consider how the impact of incarceration *unfolds over time* following confinement by estimating yearly marriage and divorce probabilities for up to 5 years following release. Although incarceration might indeed have an immediate impact on the likelihood of marriage or divorce, it is more likely that such effects take several years to accumulate. This is especially true in the case of divorce, where lengthy legal proceedings can often delay formal dissolution of the marriage. At minimum, it is an empirical question whether the effect of incarceration is felt in the short or long term.

Data and Measures

The analysis uses data from the Criminal Career and Life-Course Study (CCLS), a large-scale longitudinal study carried out at The Netherlands Institute for the Study of Crime and Law Enforcement (NSCR) (Nieuwbeerta and Blokland 2003). The CCLS is based on a representative sample of 4% of all cases of criminal offenses that were tried in The Netherlands in 1977 (the CCLS builds on the work of Block and Van der Werff 1991; Van der Werff 1986). The total sample consists of 5,164 individuals (for a recent analysis, see Blokland et al. 2005).

The criminal careers of the offenders in the sample were reconstructed using abstracts from the General Documentation Files (GDF) of the Criminal Record Office ("rap sheets"). The GDF contain information on every criminal case registered by the police at the Public Prosecutor's Office. While the GDF contain information on all offenses that have led to any type of judicial action, here we use only information on those offenses that were either followed by a conviction or a prosecutorial disposition due to policy reasons—referred to below as convictions—thereby excluding cases that resulted in an acquittal or a prosecutorial disposition due to insufficient evidence.[2] All convictions before 1977 as well as any convictions in the period 1977–2003 were recorded. For each individual in the sample we thus obtained data on the number of convictions per year starting at age 12 (the minimum age of criminal responsibility in The Netherlands) up to the year 2003.[3] When applicable the GDF extracts also contain information on the length of imprisonment following a conviction.

To obtain information on possible time-varying variables that might confound the effect of imprisonment, data on life circumstances were collected from population registration data and added to the conviction histories. Since 1938 all Dutch citizens are registered in their municipalities. Personal records in the population registration contain

[2] In the Dutch criminal justice system, the public prosecutor has the discretionary power not to prosecute every case forwarded by the police. The public prosecutor may decide to drop the case if prosecution would probably not lead to conviction due to lack of evidence or for technical considerations (e.g., procedural or technical waiver). The public prosecutor is also authorized to waive prosecution "for reasons of public interest" (i.e., waiver for policy considerations). The Board of Prosecutors-General has issued national prosecution guidelines under which a public prosecutor may decide to waive a case for policy reasons, for example, if measures other than penal sanctions are preferable or more effective, prosecution would be disproportionately unjust or ineffective in relation to the nature of the offense or the offender, or prosecution would be contrary to the interest of the state or the victim (Tak 2003).

[3] Note that in The Netherlands, a person is not given a "clean slate" upon becoming an adult. The extracts used thus contain information on both juvenile and adult offenses.

information on marriage and fertility history as well as date of death. Prior to electronic registration (that is, prior to 1994), personal record cards were used that were sent to the next town of residence every time a person moved. For individuals who had died before 1994 these personal record cards were retrieved from the Centre for Genealogy and Heraldry. Based on personal details from 1977, we were able to trace 89.4% of the original sample. Over one-half of those we were unable to trace were neither born nor residing in The Netherlands in 1977, leaving a total of 4,615 individuals in the sample to be analyzed.

For various reasons we restrict our attention to a subsample of the 4,615 individuals for whom we have life-history data. First, because of the small number of women in the dataset, and the even smaller number of women who were imprisoned, we excluded all women from consideration ($N = 424$). Second, we limited the analysis to persons who up to age 18 had not been imprisoned, a criterion that led to the exclusion of another 342 men. We made this decision because marriage is non-existent in the CCLS prior to this age. Third, we excluded the 17 individuals who died within 5 years subsequent to their year of first imprisonment. Fourth, as expounded below we contrasted those imprisoned for the first time with those convicted but not (yet) imprisoned. We thus limited our sample to persons who had at least one conviction between ages 18 and 38, excluding about a third of the remaining men. These sample restrictions result in an analysis sample of 2,790 individuals who contribute 5,264 person-years of criminal conviction information.

First-Time Imprisonment

Data on imprisonment were culled from the GDF extracts. All sentences involving detention irrespective of their length were counted as imprisonment. Of all males for whom data on life circumstances were available, 57.4% were sentenced to imprisonment at least once during their offending careers up to calendar year 2003. Offenders were imprisoned for the first time as young as age 12, with a mean age of first imprisonment of 23.4 years and a median of 21 years (the interquartile range is 19–26 years). The length of first imprisonment ranges from 1 day to 42 months, with an average of 14 weeks or about three-and-a-half months.[4]

The decision to make estimation of the incarceration effect between ages 18 and 38 our primary focus was influenced by two primary considerations. One was that a sizable number of individuals were first imprisoned during this age span. As shown in Fig. 1, 80.7% of all imprisoned persons were first imprisoned between 18 and 38 years of age. Second, we were interested in examining the impact of adult imprisonment on the likelihood of marriage, leading us to choose the age of majority (18) as the starting point for the accumulation of a prison record. This refers to the age at which offenders are brought before the adult court rather than the juvenile court. However, we take advantage of information on criminal convictions prior to age 18 in predicting the likelihood of imprisonment from that age forward.

4 While the penal regime in The Netherlands has become harsher over the years, still more than 80% of the unsuspended custodial sanctions imposed in 2007 (the most recent figure available from Statistics Netherland) were shorter than 12 months. Similar percentages have been reported in many other European countries such as Belgium, Denmark, Finland, France, Italy, Norway, Sweden, and Switzerland (see Aebi et al. 2006).

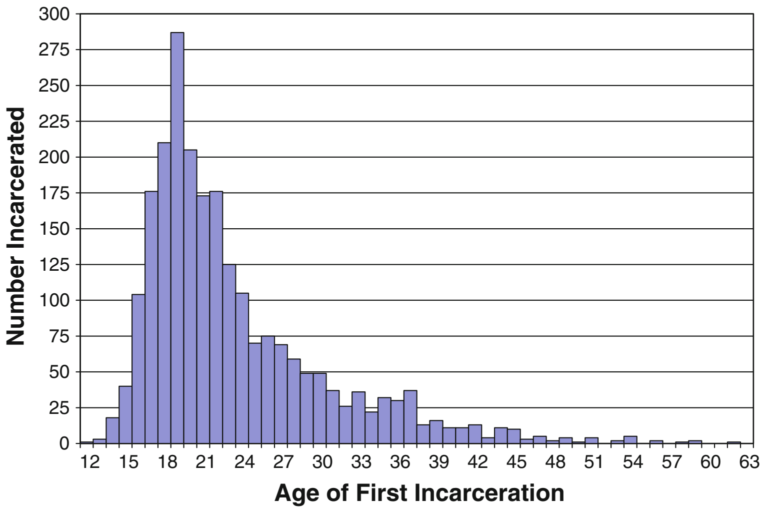

Fig. 1 Age of first incarceration in a Dutch conviction cohort. *Note*: The figure includes the 2,335 men who were ever incarcerated up to 2003. *Source*: Criminal Career and Life-Course Study (CCLS)

Empirical Methods

Our interest in this study is in the effect of first-time incarceration on the likelihood of marriage and divorce. We investigate the likelihood of marriage among individuals who were unmarried in the year they entered prison, and the likelihood of divorce among individuals who were married in the year they entered prison. In each case, the follow-up window includes the one, three, and five calendar years following incarceration. As a point of departure in describing our statistical methodology, we employ the language of program evaluation in which we are interested in estimating a "treatment effect" of incarceration on marriage and divorce.[5] A "treated" individual in our study is one who is sentenced to a spell of incarceration of any length for the first time at a given age, while an "untreated" individual is one who is observationally equivalent, does not have a history of imprisonment, and who was convicted but not imprisoned at the same age his "treated" counterpart was incarcerated.

Propensity Score Estimation

In order to model non-random selection into treatment, we employ the method of propensity score matching, and in particular the technique known as risk set matching. Rooted in the work of Rosenbaum and Rubin (1983, 1984, 1985), a propensity score is defined as "the conditional probability of assignment to a particular treatment given a vector of observed covariates" (Rosenbaum and Rubin 1983, p. 41). It is equivalent to a *balancing score* whereby the conditional distribution of observables, given the

[5] In the present context, "treatment" is generically taken to mean any intentional intervention. It is not to be confused with participation in a correctional rehabilitation program.

propensity score, is the same for imprisoned and non-imprisoned individuals.[6] The essence of covariate balance lies in the fact that, given the propensity score and suitable matches, the covariates are of no further use in predicting which of two matched individuals is imprisoned. As a result, there is no systematic tendency for the two matched individuals to be different on the basis of observables. In this analysis, the propensity score is defined as the conditional probability of imprisonment at a specific age, given current conviction for a criminal offense and no previous incarceration spell.

Risk set matching is a generalization of propensity score matching that allows the estimated propensity score to be age-dependent (Li et al. 2001).[7] Specifically, the usual propensity score is substituted by the hazard (measured in discrete time) of incarceration at age t for each individual:

$$h_i(t) = \Pr(T_i = t | T_i \geq t)$$

The hazard represents the probability that individual i experiences incarceration at time (age) t given no prior incarceration experience, where T_i signifies the individual's event time. The cumulative logistic function is then used to model the hazard of first-time imprisonment as a function of observed variables:

$$\ln\left[\frac{h_i(t)}{1 - h_i(t)}\right] = \alpha_t + \beta w_i + \delta x_{it}$$

where w_i includes time-invariant covariates measured prior to age 18, x_{it} includes time-varying covariates measured up to age t, and α_t represent age-specific constants. Individual event times are censored either because of death or post-2003 calendar year.

For clarity, note that the method used here entails matching an individual who was incarcerated at age t with an observationally similar individual who was not incarcerated at age t but was "at risk" of being incarcerated at age t (Li et al. 2001). Therefore, an individual is in the risk set only if he was at least convicted of a crime at age t, simply because a non-convicted individual is not truly at risk of incarceration. Moreover, because matching takes place on past and current rather than future observables, an individual who was incarcerated at age t is matched to an individual who was not yet incarcerated by age t, as opposed to an individual who was never incarcerated (Li et al. 2001). Thus we avoid using future information to determine

[6] Propensity score matching is similar to standard regression in that it is assumed in both cases that selection into treatment is random conditional on observed characteristics. However, propensity score matching differs from regression in at least two key ways. First, it does not rely on a linear functional form to estimate treatment effects. Although the propensity score is estimated using a parametric model, once obtained, individuals are matched non-parametrically. Second, propensity score matching highlights the issue of common support by revealing the degree to which untreated cases resemble the treated cases on observed characteristics. Standard regression (a.k.a. covariate adjustment), on the other hand, obscures this issue and risks extrapolating treatment effect estimates based solely on functional form when treated and untreated groups are actually incomparable. In many applications, only a subset of the untreated population (and perhaps the treated population as well) will be useful for estimating treatment effects. For recent empirical work employing propensity score matching, see Morgan (2001) and Harding (2003).

[7] Risk set matching is only one technique that allows the propensity score to vary as a function of age- or time-dependent covariates, a recent application of which is provided by Lu (2005). A similar goal is achieved by inverse probability-of-treatment weighting (IPTW), which is in the class of marginal structural models discussed by Robins (1999) and Robins et al. (2000). For a recent application of this approach, see Sampson et al. (2006).

eligibility for the pool of potential matched, non-imprisoned subjects.[8] This also means that an individual who is not incarcerated at age t could very well be incarcerated at some later age $t' > t$. In short, *untreated* in this study should be taken to mean *not yet treated* rather than *never treated*.

A key element in the success of the matching procedure in warding off possible selection effects is that the propensity score model includes those predictors of treatment most likely to confound the outcome of interest (Dehejia and Wahba 1999; Heckman et al. 1998). From sentencing research we know that characteristics of the instant offense, the criminal history of the offender, and his personal characteristics and life circumstances play an important role in judges' decisions (Johnson 2006). These predictors also feature prominently in actuarial risk assessment tools assessing offender dangerousness, thereby further evidencing their salience when thinking about community protection, one of the focal concerns of sentencing (see Steffensmeier and Demuth 2000). We use a variety of these measures to model the propensity score of first-time incarceration conditional on conviction.

Furthermore, our decision to restrict attention to individuals who are at least convicted of a crime at age t is intended to balance a wide variety of unobserved variables that might constitute "extralegal" factors in criminal justice processing. To the degree that latter stages of processing afford criminal justice officials less discretion in the determination of offender dispositions, concern over biased estimates of the effect of incarceration is mitigated. Nevertheless, we conduct a sensitivity analysis to ascertain how much bias from unobservables would be necessary to undermine our treatment effect estimates.

Matching Protocol and Treatment Effect Estimation

With the hazard of imprisonment in hand, we match each incarcerated individual with his same-aged, nearest neighbor, with replacement and within a caliper set to 0.001.[9] Estimation of the average treatment effect (ATE) of incarceration then proceeds in a straightforward manner. The treatment effect for each imprisoned individual is his post-release outcome less the mean outcome for his matched, non-imprisoned counterpart. The sample ATE at each post-incarceration age is then recovered by taking the expectation over each of the $i = 1,\ldots,N$ incarcerated subjects and his matched, non-incarcerated counterpart indexed by i,j:

[8] The underlying rationale for this approach is well argued by Li et al. (2001) in their hypothetical example of a clinical trial:

> Imagine a strict rule that assigned patients to treatment whenever their symptoms became acute. In this hypothetical case, to know that a patient never received treatment is to know that the patient had a relatively favorable outcome. If the control group consisted of all patients who never received treatment, then it would contain only patients with favorable outcomes, because any patient whose symptoms later became acute received the treatment. (Li et al., 2001, p. 871)

Simply put, untreated subjects in this hypothetical scenario were never truly at risk of being treated. In the language of program evaluation, treatment is not independent of potential outcomes. Such an after-the-fact selection rule would obviously introduce extreme biases into any treatment effect estimates.

[9] We employed a wide range of matching protocols and achieved results that were substantially similar. The results we display are from 1-to-1 matching with replacement and a caliper of 0.001. In sensitivity analyses, we matched each treated individual with one, three, and five nearest neighbors within calipers of 0.05, 0.01, and 0.001.

$$\mathrm{ATE} = \frac{1}{N}\sum_{i=1}^{N}\left(y_i - y_{i,j}\right) = \bar{y}_i - \bar{y}_{i,j}$$

In fact, this quantity represents the *average treatment effect on the treated*. The outcome y (subscripted by i or i,j depending on whether the indexed subject is treated or untreated) is one of two binary indicators: An indicator for marriage conditional on being unmarried at the time of treatment, and an indicator for divorce conditional on being married at the time of treatment. In order to take into account sampling variability, the variance of the treatment effect (VTE) is estimated by:

$$\mathrm{VTE} = \frac{1}{N}\left(s_i^2 + s_{i,j}^2\right)$$

where s_i and $s_{i,j}$ are the sample standard deviations of y_i and $y_{i,j}$, respectively. For the purpose of statistical inference, the standard error is obtained by taking the square root of the variance.

To provide some sense of the substantive (in addition to statistical) significance of the impact of imprisonment, we provide an estimate of the standardized difference as described by Rosenbaum and Rubin (1985).[10] This quantity is equivalent to and serves the same function as Cohen's d (Cohen 1988), a common measure of effect size. Our estimate of the standardized difference, or what we refer to below as the effect size (ES), is calculated:

$$\mathrm{ES} = \frac{\bar{y}_i - \bar{y}_{i,j}}{\sqrt{\frac{s_i^2 + s_j^2}{2}}}$$

The means are indexed by i and i,j, signifying the mean of y for incarcerated individuals less the mean for their matched, non-incarcerated counterparts—the average treatment effect (ATE). However, in the denominator, the standard deviations are indexed by i and j, denoting the standard deviation of y for incarcerated individuals and the standard deviation of y for all non-incarcerated individuals, whether they are matched or not. The effect size serves as a supplement to our tests for statistical significance when low statistical power is a consideration. Following convention (Cohen 1988; Rosenbaum and Rubin 1985), the rule of thumb that we use to judge whether an average treatment effect is substantively meaningful is $|\mathrm{ES}| \geq 0.20$.

Results

As a point of departure, Fig. 2 illustrates cumulative and contemporaneous marriage probabilities in the CCLS among all men with no prison record until 2003 compared to men with at least one incarceration spell. Inspection of the cumulative probability of marriage shows a gap in the likelihood of ever being married that is a function of imprisonment as well as age. Although incarcerated men are somewhat more precocious in the transition to marriage until the early-20s, a gap in the likelihood of marriage emerges

[10] As the formula shows, the standardized difference provides an estimate of the mean difference as a proportion of the average standard deviation. Rosenbaum and Rubin (1985) proposed using the standardized difference to judge covariate balance before and after matching on the propensity score, which we do in the appendix. However, the measure also has utility for estimating an effect size for the average treatment effect.

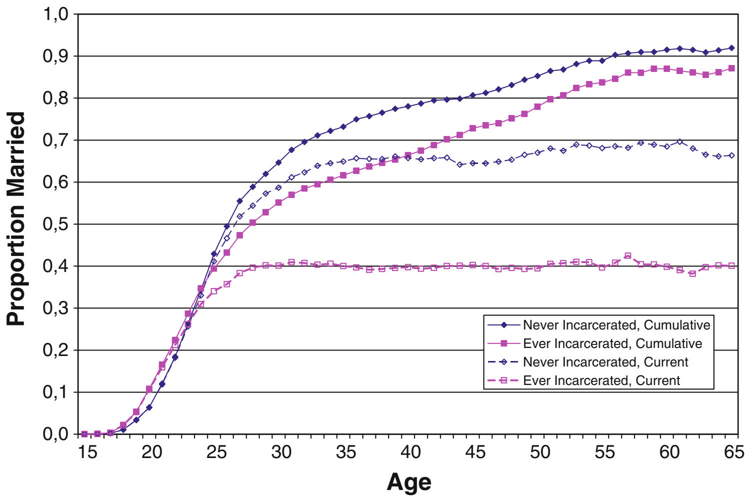

Fig. 2 Current and cumulative marriage probabilities, by imprisonment status. *Note*: The figure reports on the 1,856 men who were never incarcerated up to 2003 and the 2,335 men who were ever incarcerated. *Source*: Criminal Career and Life-Course Study (CCLS)

from the mid-20s onward, which happens to correspond with the mean age of first imprisonment (23.4 years). This gap appears to narrow with age, however, and to stabilize at about five percentage points.

Differences in the probability of currently being married are far more notable than differences in the probability of having ever been married. By age 30 and later, about 40% of ever-imprisoned men are currently married, a figure that remains stable at least until the mid-60s. Among never-imprisoned men, on the other hand, the corresponding figure is about 66%. This means that ever-imprisoned men have a current marriage likelihood that is about 39% lower than never-imprisoned men. Recall that all of the men in the CCLS are convicted at some point (in 1977, at a minimum), so the sample can be considered high risk. Thus the difference is surprisingly large considering that we have a "homogeneous" sample.

Fig. 2 thus provides prima facie evidence for the disruptive impact of incarceration on marriage. However, its effect on existing marriages (via divorce) seems far more salient, and there is less compelling evidence for long-term reductions in the likelihood of a transition to marriage. In fact, imprisonment would appear to have a predominantly incapacitative effect on transitions to marriage, as indicated by the wider gap in cumulative marriage probabilities in the 30s and 40s (the prison-prone years), but with only a small permanent reduction that persists into later life on the order of about 5%. However, it is unclear whether these observed differences can be plausibly attributed to the causal effect of imprisonment or to the differential selection of imprisoned offenders into non-marriage and, in particular, unstable marital situations.

Risk Set Matching Model of First-Time Imprisonment

As a first step toward causal inference, we estimate a propensity score of incarceration. Table 1 provides the results from the discrete-time hazard model of first-time

Table 1 Discrete-time hazard model of first-time imprisonment conditional on conviction

	Mean (SD)	Bivariate coefficients		Multivariate coefficients	
		b (SE)	exp(*b*)	*b* (SE)	exp(*b*)
Instant offense					
Category of offense					
Simple theft (ref.)	15.3%	–		–	
Rape	1.7%	1.993 (0.238)***	7.34	2.413 (0.256)***	11.17
Sexual assault	2.0%	0.983 (0.217)***	2.67	1.275 (0.235)***	3.58
Sexual abuses/other sexual offense	3.1%	0.555 (0.190)**	1.74	0.664 (0.205)**	1.94
Threatening	2.4%	0.873 (0.204)***	2.39	0.961 (0.226)***	2.61
(Attempted) murder/ manslaughter	1.1%	1.118 (0.282)***	3.06	1.371 (0.327)***	3.94
Assault	16.3%	0.362 (0.116)**	1.44	0.386 (0.126)**	1.47
Violent theft	1.9%	2.327 (0.241)***	10.25	2.469 (0.262)***	11.82
Extortion	0.6%	2.180 (0.408)***	8.85	2.277 (0.445)***	9.75
Forgery	2.4%	0.060 (0.231)	1.06	0.084 (0.271)	1.09
Aggravated assault	23.6%	0.839 (0.105)***	2.31	0.793 (0.115)***	2.21
Embezzlement	1.8%	0.038 (0.267)	1.04	−0.087 (0.286)	0.92
Swindling	0.8%	0.064 (0.387)	1.07	0.006 (0.417)	1.01
Fencing	3.6%	−0.074 (0.203)	0.93	−0.040 (0.216)	0.96
Public order	3.2%	−0.098 (0.212)	0.91	0.107 (0.224)	1.11
General offending	0.6%	1.007 (0.374)**	2.74	1.321 (0.398)***	3.75
Offense against authority	2.3%	−0.719 (0.297)*	0.49	−0.566 (0.309)†	0.57
Flashing	4.1%	−1.592 (0.321)***	0.20	−1.540 (0.339)***	0.21
Vandalism	6.1%	−1.225 (0.233)***	0.29	−1.045 (0.241)***	0.35
Other criminal law	1.1%	−0.990 (0.477)*	0.37	−1.266 (0.508)*	0.28
Opium act	4.1%	0.377 (0.175)*	1.46	0.556 (0.192)**	1.74
Weapons act	2.1%	0.210 (0.238)	1.23	0.681 (0.253)**	1.98
Severity of offense	0.017 (1.38)	0.189 (0.024)***	1.21	0.210 (0.027)***	1.23
No. crimes in case of conviction					
Violent crimes	0.020 (0.16)	0.983 (0.188)***	2.67	0.493 (0.203)*	1.64
Property crimes	0.090 (0.36)	1.141 (0.090)***	3.13	0.905 (0.098)***	2.47
Other crimes	0.064 (0.28)	0.547 (0.102)***	1.73	0.667 (0.117)***	1.95
Criminal history					
No. crimes in past 5 years					
Violent crimes	0.192 (0.51)	0.060 (0.059)***	1.06	0.012 (0.069)	1.01
Property crimes	0.668 (1.18)	0.249 (0.025)***	1.28	0.237 (0.031)***	1.27
Other crimes	0.313 (0.75)	0.021 (0.040)	1.02	0.144 (0.050)**	1.15
No. crimes beyond past 5 years					
Violent crimes	0.125 (0.50)	−0.381 (0.085)***	0.68	−0.164 (0.098)†	0.99
Property crimes	0.374 (0.97)	−0.125 (0.036)***	0.88	−0.009 (0.048)	0.85
Other crimes	0.147 (0.57)	−0.320 (0.073)***	0.73	0.086 (0.079)	1.09
Early conviction (before age 16)	24.4%	0.228 (0.070)***	1.25	0.074 (0.092)	1.08

Table 1 continued

	Mean (SD)	Bivariate coefficients		Multivariate coefficients	
		b (SE)	exp(b)	b (SE)	exp(b)
Life circumstances					
Current marital and fertility status					
Not married—no children (ref.)	69.2%	–		–	
Not married—children	2.9%	−0.061 (0.182)	0.94	0.318 (0.254)	1.37
Married—no children	6.4%	−0.690 (0.147)***	0.50	−0.424 (0.189)*	0.65
Married—children	16.9%	−0.299 (0.086)***	0.74	0.160 (0.191)	1.17
Separated—no children	1.1%	−0.475 (0.327)	0.62	−0.023 (0.392)	0.98
Separated—children	3.4%	−0.101 (0.170)	0.90	0.372 (0.262)	1.45
Marital and fertility history					
No. years married in past 5 years	86.0%	−0.094 (0.020)***	0.91	−0.025 (0.052)	0.97
No. years have children in past 5 years	78.6%	−0.062 (0.019)***	0.94	−0.005 (0.051)	0.99
Sociodemographics					
Non-Dutch	12.8%	0.647 (0.085)***	1.91	0.606 (0.101)***	1.83
Birth cohort					
Cohort 1	36.7%	−0.464 (0.084)***	0.63	−1.616 (0.114)***	0.20
Cohort 2	42.7%	−0.199 (0.079)*	0.82	−0.897 (0.097)***	0.41
Cohort 3 (ref.)	20.6%	–		–	

$N = 2{,}790$; $NT = 5{,}264$. The age dummies and a constant are suppressed to conserve space

† $p < .10$, * $p < .05$, ** $p < .01$, *** $p < .001$ (two-tailed tests)

imprisonment at age t. These are logistic coefficients that, when logged, represent hazard ratios.[11] The model is based on 5,264 person-periods for 2,790 male offenders convicted between the ages of 18 and 38. Recall that our strategy entails conditioning on conviction at each age. The coefficients thus represent the predictive influence of the listed characteristics on first-time imprisonment risk given that an individual has been convicted and is therefore at risk of confinement.[12]

The results in Table 1 are closely aligned with existing research on the factors that influence the receipt of custodial sentences. Collectively, the most salient determinants of incarceration are official characteristics that summarize the severity of the current offense and the offender's recent criminal history. The first set of predictors describes the

[11] The transformation, $\exp(b) - 1$, provides the proportional increase/decrease in the hazard of imprisonment given a one-unit increase in the explanatory variable, evaluated at the means of the remaining covariates.

[12] Note that the hazard model is estimated for the entire eligible sample (i.e., individuals convicted at age t with no previous incarceration spell), including those who are married as well as unmarried at the time of their conviction/incarceration. We control for marital status at the time of conviction in the model. We pool both samples together in the propensity score model because there is no reason to believe that the incarceration mechanism differs qualitatively for married and unmarried men. Then, in order to estimate the average treatment effect (ATE) of incarceration, we stratify the sample directly by the offender's marital status. The strategy of estimation within subpopulations defined by the covariates has precedence in the literature on the propensity score methodology (Rosenbaum and Rubin 1984).

characteristics of the "instant offense," or the offense(s) for which an individual is convicted at age t. For each conviction the underlying section of the criminal code was used to distinguish between types of offenses, and in offenses leading to multiple convictions, the offense type was top-coded. Offenses of rape, violent theft, and extortion carry a far higher odds (hazard) of imprisonment than simple theft (the reference offense), followed by sexual assault, (attempted) homicide,[13] and "general offending." The lowest imprisonment risk is observed for individuals convicted of such offenses as flashing, vandalism, and "other criminal law" offenses. A continuous measure of offense severity—representing the maximum penalty prescribed in the penal code—is positively and significantly associated with incarceration, as would be expected. Continuous measures of the number of crimes underlying the current conviction also have positive coefficients, indicating that criminal cases with multiple offenses (i.e., "multiple counts") are significantly more likely to result in imprisonment.

The second set of characteristics summarizes the criminal history of the offender. These include the number of convictions in the 5 years prior to the current conviction as well as the number of convictions in the remainder of the criminal career beyond those 5 years. As shown, individuals with a more extensive criminal history are more likely to be incarcerated, particularly if a criminal history has been accumulated in the past 5 years. Convictions beyond this period are largely unrelated to imprisonment risk. A dichotomous indicator of conviction prior to age 16 is unrelated to incarceration once other criminal history measures are controlled.

The final set of predictors summarizes the life circumstances and sociodemographic characteristics of the offenders. Although life circumstances are largely unrelated to confinement in the multivariate model, the non-Dutch are significantly more likely to be incarcerated net of legally relevant factors. Additionally, there are period effects as indicated by the birth cohort indicators, signifying greater punitiveness over time in The Netherlands. Although age dummies are not shown in Table 1, the hazard of imprisonment rises and then falls as individuals age, with the peak hazard observed at ages 19–22.[14]

Effect of First-Time Imprisonment on the Likelihood of Marriage

Table 2 provides estimates of the effect of incarceration on the likelihood of marriage among men who were not married in the year of conviction. In order to ascertain how (if) the effect of imprisonment on the likelihood of marriage unfolds over time, we provide treatment effect estimates for 1, 3, and 5 years following first-time imprisonment. The first row of coefficients compares marriage probabilities of imprisoned men with all non-imprisoned men in the CCLS. In all three time periods, the difference is statistically significant and grows by about 1.5 probability points every 2 years. When age dummies are controlled (the second row of coefficients), the differences remain statistically significant. Although statistically significant, however, the magnitude of these differences are quite small as indicated by the effect size estimates—at maximum 0.10, or 10% of the sample standard deviation, which is not noteworthy by any conventional standard.

[13] Note that most of the offenses labeled as attempted manslaughter in The Netherlands would be classified as assaults in the US.

[14] Comparing the covariates listed in Table 1 for the treated and untreated before and after conditioning on the propensity score shows that, before matching, there is imbalance on a number of covariates. In particular, using mean comparisons (t-tests) about half of the covariates are imbalanced, while using effect sizes about one-quarter are imbalanced. After matching, however, there is (almost) complete balance. Details on covariate balance are provided in the appendix.

Table 2 Average treatment effect of imprisonment on post-release marriage probabilities

Matching protocol	1 Year post-incarceration		3 Years post-incarceration		5 Years post-incarceration	
	ATE (SE)	ES	ATE (SE)	ES	ATE (SE)	ES
Unmatched, all men						
Unadjusted	−.0194 (.0064)**	−.08	−.0343 (.0112)**	−.09	−.0474 (.0136)***	−.10
Age adjusted	−.0183 (.0065)**	−.08	−.0427 (.0111)***	−.11	−.0706 (.0134)***	−.16
Unmatched, convicted men						
Unadjusted	−.0047 (.0076)	−.02	−.0035 (.0126)	−.01	.0003 (.0150)	+.00
Age adjusted	−.0061 (.0076)	−.03	.0001 (.0127)	+.00	−.0084 (.0151)	−.02
Nearest neighbor matching	−.0206 (.0140)	−.09	−.0038 (.0218)	−.01	−.0093 (.0269)	−.02

Point estimates represent the average treatment effect on the treated, with cluster-robust standard errors in parentheses. Nearest neighbor matching is 1-to-1 with replacement and within a 0.001-caliper

ATE average treatment effect (on the treated), *ES* effect size or standardized difference (Cohen's *d*)

† $p < .10$, * $p < .05$, ** $p < .01$, *** $p < .001$ (two-tailed tests)

The results when using all non-imprisoned men as a comparison sample harmonizes with existing research on the effect of incarceration on marriage. However, the problem of stochastic selectivity (a kind of selection bias) complicates interpretation of the observed marriage differences as causal. Therefore, the second set of coefficients compares marriage probabilities of imprisoned men with all non-imprisoned men who were also convicted. Thus, the counterfactual marriage probability is derived from a high-risk sample of men who were filtered almost as far through the criminal justice system as the imprisoned men. Interestingly, conditioning the sample on conviction in this way reduces all treatment effects to statistical and substantive non-significance.

The final set of coefficients matches each imprisoned (treated) offender with a same-aged convicted (untreated) counterpart, thereby choosing from among an already high-risk comparison sample only those who most closely resemble the imprisoned individuals on the observable characteristics shown in Table 1. Only 84.7% of the imprisoned men have valid matches, or in propensity score (risk set) matching parlance, are "on support."[15] One year post-incarceration, the treatment effect is 2.06 probability points but fails to achieve statistical significance ($p < .140$). A logistic regression of 1-year marriage on incarceration produces an odds ratio of 0.698 ($b = -0.360$), which implies a 30.2-percent drop in the odds of marriage ($0.698-1 = -0.302$) 1 year following first-time imprisonment ($p < .119$). In subsequent time periods, however, the treatment effect is so miniscule as to be indistinguishable from zero. Thus, the strongest evidence for an impact of incarceration on marriage is the first year post-imprisonment. Yet even this evidence is weak, as judged by the lack of statistical significance and the small effect size (−0.09).

[15] Imprisoned men for whom we were unsuccessful in identifying suitable matches tended to be convicted of comparatively more serious offenses and to have a more extensive criminal history. For example, they were more likely to be convicted of rape or violent theft, to have more crimes involved in their conviction offense, and to have more prior property convictions.

Table 3 Average Treatment Effect of Imprisonment on Post-Release Divorce Probabilities

Matching protocol	1 Year post-incarceration		3 Years post-incarceration		5 Years post-incarceration	
	ATE (SE)	ES	ATE (SE)	ES	ATE (SE)	ES
Unmatched, all men						
Unadjusted	.0629 (.0170)***	+.28	.1324 (.0243)***	+.39	.2021 (.0282)***	+.50
Age adjusted	.0618 (.0172)***	+.27	.1235 (.0244)***	+.36	.1824 (.0279)***	+.45
Unmatched, convicted men						
Unadjusted	.0285 (.0187)	+.11	.0540 (.0270)*	+.14	.0922 (.0312)**	+.21
Age adjusted	.0369 (.0194)†	+.14	.0533 (.0279)*	+.14	.0821 (.0318)**	+.18
Nearest neighbor matching	.0568 (.0291)†	+.22	.1036 (.0411)*	+.27	.1390 (.0501)**	+.31

Point estimates represent the average treatment effect on the treated, with cluster-robust standard errors in parentheses. Nearest neighbor matching is 1-to-1 with replacement and within a 0.001-caliper

ATE average treatment effect (on the treated), *ES* effect size or standardized difference (Cohen's *d*)

† $p < .10$, * $p < .05$, ** $p < .01$, *** $p < .001$ (two-tailed tests)

Effect of First-Time Imprisonment on the Likelihood of Divorce

Table 3 provides estimates of the effect of incarceration on the likelihood of divorce among men who were married in the year of conviction. As before, we provide 1-, 3-, and 5-year divorce probability differences. The first set of coefficients treats the divorce likelihood of all non-imprisoned men (whether they are convicted at age t or not) as the counterfactual outcome. These results reveal a substantial impact of imprisonment on divorce in all time periods following release, which is only modestly attenuated when age dummies are controlled. Moreover, the impact grows considerably as time elapses. For example, by the fifth year following release, imprisoned men have a divorce probability that is an astonishing 20.21 points higher than non-imprisoned men.

The second set of coefficients includes only men who were convicted of a crime in the comparison sample. Doing so reduces the magnitude of the treatment effects by half, although they still achieve statistical significance in post-release years three and five, and the effect is still substantively meaningful in the fifth post-imprisonment year, with an effect size of 0.21. Direct adjustment for age does little to change the substantive findings.

In the final set of results, the treatment effects after matching each imprisoned individual with his same-aged nearest neighbor are shown. In this analysis, only 64.0% of imprisoned individuals have valid matches and thus contribute to the estimate of the treatment effects.[16] The differences in divorce probabilities are 0.0568, 0.1036, and 0.1390 after 1, 3, and 5 years following imprisonment, respectively. Not only are these results statistically significant (or at least marginally so 1 year post-incarceration), the effects sizes all exceed 0.20, which we interpret as noteworthy. Additionally, a logistic regression of the likelihood of divorce after 5 years produces an odds ratio of 2.096 ($b = 0.740$), which implies a full doubling of the odds of divorce that is attributable to incarceration ($p < .009$).

[16] The imprisoned men who were off support were more likely to be convicted of violent offenses (e.g., rape, violent theft, aggravated assault) and less likely to be convicted of property offenses (e.g., theft, forgery, weapons act), had a higher offense severity score and more extensive criminal histories, and were convicted for the first time prior to age 16. They were also more likely to be ethnic minorities.

Subgroup Estimates of the Impact of Incarceration

Having estimated the overall impact of first-time incarceration on marriage formation and stability, we proceed with subgroup analyses along a number of measurable dimensions. For ease of presentation, we limit our attention to the 5-year effects of incarceration. One pertinent question is the degree to which the impact of incarceration varies by the age at which an individual is imprisoned for the first time. To test this possibility, we first created an interaction term representing the product of the offender's age with the imprisonment indicator. We also conducted nearest neighbor matching within 2-year age intervals and estimated age-specific treatment effects. In all instances, however, there was no discernible pattern.[17]

Second, we created interaction terms between the imprisonment indicator and several of the life circumstances, sociodemographic indicators, and offense characteristics. Most of the tests for interactions failed to reveal different effects of imprisonment status on the likelihood of marriage and divorce (e.g., length of marriage, ethnicity), although we acknowledge that statistical power was potentially compromised in many of these tests. However, there were several notable findings in the models for divorce. Men who were married with children were far less likely to get divorced as a result of imprisonment than married men without children (the interaction term is significant at two-tailed $p < .001$). For example, the probability of divorce among imprisoned men with children is 0.27, compared to 0.51 among imprisoned men with no children.[18]

Divorce risk is also higher among men imprisoned for more serious offenses, as indicated by an interaction term between imprisonment and our continuous measure of offense severity (significant at two-tailed $p < .047$). Although offense severity exerts no influence on divorce among men who are only convicted for a crime, imprisonment for a more serious offense leads to higher divorce risk compared to imprisonment for a less serious offense.

Sensitivity of the Average Treatment Effects to Hidden Bias

Finally, we conduct a sensitivity analysis of the propensity score estimates of the effect of first-time imprisonment. This analysis proceeds along the lines recommended by Rosenbaum (2002). If the probability that an individual receives treatment is a function of the observed covariates and the observed covariates alone, then a study is free of hidden bias. However, Rosenbaum observes: "There is *hidden bias* if two units with the same observed covariates **x** have differing chances of receiving the treatment" (Rosenbaum 2002, p. 106, emphasis in original). This is to say that, if the propensity score model employed in this analysis is not entirely successful in capturing selection into the treatment, the estimated treatment effects may still be compromised. We therefore evaluate the sensitivity of the average treatment effect to a range of assumptions concerning the presence and magnitude of unobserved confounding.[19] Details on the procedure we use are provided in an attached appendix. To economize the presentation of results, we limit our attention to the 5-year effects. By way of review, the average treatment effect on marriage is –0.0093 ($p < .728$)

[17] Results are not shown but are available upon request.

[18] Interestingly, the effect of children appears to be opposite (but not quite marginally so) for convicted but non-imprisoned men: Convicted men with children have a divorce probability of 0.21 compared to 0.11 among convicted men without children ($p < .107$).

[19] To conduct the analysis, we invoke the user-written Stata routine—mhbounds—developed by Becker and Caliendo (2007).

Table 4 Sensitivity of 5-year treatment effect estimates to hidden bias

Gamma (Γ)	Effect of incarceration on marriage		Effect of incarceration on divorce	
	Q^+ (*p*-value)	Q^- (*p*-value)	Q^+ (*p*-value)	Q^- (*p*-value)
1.00	−0.23 (.407)	−0.23 (.407)	+2.72 (.003)	+2.72 (.003)
1.05	−0.66 (.254)	−0.08 (.469)	+2.54 (.006)	+2.92 (.002)
1.10	−1.07 (.143)	+0.48 (.314)	+2.36 (.009)	+3.10 (.001)
1.15	−1.46 (.073)	+0.87 (.192)	+2.19 (.014)	+3.27 (.001)
1.20	−1.83 (.034)	+1.24 (.107)	+2.03 (.021)	+3.44 (.000)
1.25	−2.19 (.014)	+1.60 (.055)	+1.87 (.031)	+3.60 (.000)
1.30	−2.53 (.006)	+1.94 (.026)	+1.72 (.042)	+3.75 (.000)
1.35	−2.86 (.002)	+2.28 (.011)	+1.58 (.057)	+3.90 (.000)
1.40	−3.18 (.001)	+2.59 (.005)	+1.44 (.075)	+4.04 (.000)
1.45	−3.49 (.000)	+2.90 (.002)	+1.31 (.095)	+4.18 (.000)
1.50	−3.79 (.000)	+3.20 (.001)	+1.18 (.119)	+4.32 (.000)

Γ signifies the odds ratio for the effect of unobservables on the likelihood of imprisonment for individuals who were actually imprisoned versus individuals who were not imprisoned. Refer to the appendix for details. Q represents Mantel and Haenszel (1959) test statistic, which is distributed as a standard normal random variable (i.e., it is a z-test). Q^+ signifies the simulated test statistic under the assumption that the treatment effect is overestimated (i.e., the propensity score matching estimate is biased by positive self-selection), while Q^- signifies the simulated test statistic under the assumption that the treatment effect is underestimated (i.e., the propensity score matching estimate is biased by negative self-selection). One-tailed *p*-values are provided for each statistic

from Table 2, and the average treatment effect on divorce is 0.1390 ($p < .006$) from Table 3. The sensitivity analysis is based on Mantel and Haenzel's (1959) test statistic, Q, for binary outcomes (see also Aakvik 2001; Becker and Caliendo 2007).

The results of the estimation of "Rosenbaum bounds" on the effect of incarceration are displayed in Table 4. The first row of coefficients provides test statistics and corresponding *p*-values for $\Gamma = 1.0$, which is equivalent to the scenario of no hidden bias. These estimates replicate the substantive and statistical findings from Tables 2 and 3. Specifically, imprisonment has no effect on the likelihood of marriage ($Q = -0.23$, two-tailed $p < .814$) and increases the likelihood of divorce ($Q = 2.72$, two-tailed $p < .006$). The coefficients in the remaining rows evaluate the sensitivity of these effects (or their absence) to the degree to which they may be overestimated or underestimated because of unobserved confounding. For example, a value of $\Gamma = 1.2$ represents a scenario assuming that hidden bias would increase the odds of imprisonment for a treated individual compared to an untreated individual by an additional 20%, over and above the estimated propensity score. If we believe that positive self-selection exists despite conditioning on the estimated propensity score, then the coefficients and *p*-values under the Q^+ column are appropriate. On the other hand, if we believe that negative self-selection exists, then the coefficients and *p*-values under the Q^- column are appropriate.[20]

[20] It important to emphasize that this sensitivity analysis generates "worst case" bounds on estimated treatment effects. That is, even small values of Γ that result in non-significance of a treatment effect do not invalidate the original estimates. DiPrete and Gangl (2004) explain that non-significance simply means that confidence intervals for the impact of incarceration would include zero if an unobserved variable was responsible for a difference in the odds of treatment assignment between imprisoned and non-imprisoned individuals, and "if this variable's effect on [marriage or divorce] was so strong as to almost perfectly determine whether [the likelihood of marriage or divorce] would be bigger [or smaller] for the treatment or

In order to estimate an inverse relationship between incarceration and marriage that is at least marginally significant, positive self-selection on the order of $\Gamma = 1.20$ and larger would be required. In other words, if there is reason to believe that, net of the propensity score (and the covariates that comprise it), imprisoned individuals are still more likely to be married for reasons that have nothing to do with their incarceration experience, then the estimated effect of incarceration on marriage would be negative. Although such positive self-selection seems counterintuitive, recall from Fig. 2 that, at least into their 20s, men with a history of incarceration are actually more likely to be married. This gives support to the possibility that unobserved self-selection, if present, is positive and may be upwardly biasing our estimate of the impact of incarceration on marriage.[21]

The effect of incarceration on divorce is sensitive to positive self-selection on the order of $\Gamma = 1.35$ and larger, which would render the estimated effect no longer significant at a 10-percent level. However, if we have reason to believe that our treatment effects are biased by negative self-selection—that the treatment effect is underestimated—then the average treatment effect grows with the magnitude of confounding. Considering that treatment effect estimates in applied research are often sensitive to Γ as small as 1.15 (e.g., Becker and Caliendo 2007), we judge this to be a robust result.

Discussion

In this study, our interest has centered on the impact of first-time imprisonment on the likelihood of marriage and divorce. Considering the (by now) well-established protective role that marriage plays in the criminal career (in the male criminal career, at least), as well as cross-national expansion in the use of incarceration as the predominant form of crime control, an important social concern is the degree to which widespread use of prison may actually backfire by worsening the life chances of offenders returning to the community after they have paid their debt to society (Hagan and Dinovitzer 1999). If imprisoned offenders suffer deficits in the marriage market following their confinement (among other markets, most notably the labor market), then one potent avenue for the adoption of a more conventional, law-abiding lifestyle is denied them. Their exclusion from such institutional participation could lead to a delay in the desistance process and thus to prolonged criminal involvement.

To estimate the presence and magnitude of these effects, we utilized data from a long-term study of a cohort of Dutch offenders convicted of a criminal offense in 1977. We employed the technique of risk set matching, which involved identifying at least one convicted offender who was not imprisoned but was at equally high risk of imprisonment. These data and methods have several advantages for a study of the impact of imprisonment on marriage and divorce. First, we are uniquely situated to conduct a long-term follow-up (up to 5 years, in our application) to ascertain how the effect of first-time incarceration

Footnote 20 continued
the control case in each pair of matched cases in the data" (DiPrete and Gangl 2004, p. 291, emphasis removed). They explain further that, in instances where unobserved confounding strongly affects treatment assignment but only weakly influences the response variable, the estimated confidence intervals would not include zero. We would add further that, if the unobserved variable is not binary, then the Rosenbaum bounds are again too wide for a given value of Γ.

[21] If we instead estimate Rosenbaum bounds on the 1-year marriage likelihood, we find that positive self-selection on the order of $\Gamma = 1.05$ pushes the incarceration effect on marriage to 10-percent significance, while $\Gamma = 1.15$ pushes it to 5-percent significance.

unfolds over time. We can thus examine whether the impact of incarceration is immediate or delayed, and whether it decays or grows with the passage of time. Second, we avoid comparing the marriage and divorce likelihoods of imprisoned offenders to “everyone else,” which is the convention in all of the existing studies. We instead restrict our attention to individuals who have at least been convicted at the same age, which on its face provides a stronger counterfactual source because of the known stochastic selectivity of higher-risk individuals into later stages of the criminal justice system (Blumstein et al. 1993). Third, the use of risk set matching ensures that we select from this comparison sample only those convicted individuals who themselves have a high risk of being incarcerated, as determined by a number of official and unofficial characteristics known from sentencing research to predict custodial sanctions.

The results demonstrate a short-lived, but quite modest, impact of confinement on the probability of marriage. But we note that ours is a generous interpretation, as the effect is not statistically significant and can only be judged as “small” in terms of its effect size. Nevertheless, this interpretation is consistent with a number of other studies demonstrating an inverse incarceration-marriage link that exists either contemporaneously or in the very short term (Huebner 2005, 2007; Lopoo and Western 2005; Sampson et al. 2006; Western 2006; Western et al. 2004; Western and McLanahan 2000). By the time that 3 years have elapsed, however, the differences in marriage attributable to incarceration are no longer discernible when imprisoned offenders are matched with similarly high-risk convicted men. This finding conflicts with other studies that observe long-term effects (Huebner 2005, 2007; Lopoo and Western 2005; Raphael 2007; Sampson and Laub 1993; Sampson et al. 2006).

In our data, then, imprisoned offenders suffer no long-term deterioration in their marriage prospects, at least compared to convicted individuals that look similar to them. This leads us to conclude that the short-term impact of incarceration on marriage, to the extent there is one, reflects nothing more than a residual incapacitation effect—imprisonment temporarily delays nuptials but does not permanently disrupt them. Simply put, imprisoned offenders are temporarily restrained from participation in the marriage market, and therefore require some time (but no more than 1 year) to “get back into the game,” as it were. If incarceration stigmatizes offenders or otherwise constrains their opportunities in the marriage market, we would expect its impact to be felt well beyond the first year after their return to the community. The fact that we observe no such effect in the risk set matching model (and in fact, prior to matching, when we only condition on having been convicted) suggests to us that selection bias is a pernicious problem in existing studies of incarceration and marriage and that “everyone else” is an inappropriate comparison group to estimate the impact of incarceration on any outcome. With respect to the likelihood of marriage, then, our findings support the criminal propensity position that any correlation between incarceration and marriage is a selection artifact and not the causal effect of incarceration.

We must also bear in mind an additional possibility, pointed out by an anonymous reviewer. Incarceration could have a specific deterrent effect on future conviction or incarceration that might very well cancel out some of the effects observed (or rather, not observed) on marriage in our analysis. In other words, incarceration might have a direct, negative effect on the likelihood of marriage that is cancelled out by an indirect, positive effect through future conviction or future incarceration spells. Assuming that the direct and indirect effects are of approximately the same magnitude (only differing in sign), the overall effect of incarceration on marriage would be rendered null. This would happen if incarceration reduced the likelihood of subsequent conviction/incarceration, and later conviction/incarceration reduced the likelihood of marriage. While plausible in theory, a recent analysis of the specific deterrent effect of imprisonment in these data suggests that it is an unlikely possibility. Nieuwbeerta et al. (2009) conclude that imprisonment actually exacerbates criminal conviction. In fact, a recent review of the specific deterrent effect of imprisonment also suggests that this cancellation is unlikely (Nagin et al. 2009).[22] We evaluated this empirically by adjusting our estimates by conviction and incarceration during the same 1-, 3-, and 5-year periods that we consider marriage (these results are not shown). In no instances were the results changed.

In contrast to the findings for marriage, the results indicate quite convincingly that the effect of incarceration on divorce is statistically significant and substantively meaningful. This harmonizes with the few existing studies on the incarceration-divorce link (Lopoo and Western 2005; Western 2006). In our data, by the fifth year post-release, imprisoned men have a divorce probability that is 56.8% higher than comparable, convicted but non-imprisoned men. In light of our methodological approach, we are inclined to attribute this finding to the causal effect of first-time imprisonment on divorce. A sensitivity analysis ("Rosenbaum bounds") additionally demonstrates that unobserved confounding must be fairly substantial to render the incarceration–divorce relationship non-significant.

We find additional evidence from subgroup analysis that married men without children and those convicted of serious offenses experience a divorce likelihood that is even higher than their imprisoned counterparts with children or convicted of minor offenses. In our data, then, children appear to provide a means for marital stability among imprisoned men. Children perhaps constitute "marital-specific capital" and therefore increase the gains of family preservation relative to dissolution (see Becker 1973; Becker et al. 1977). On the other hand, imprisonment for a serious crime might provide a clear incentive for a spouse to withdraw from the marriage.[23]

The implications of these findings are sobering, especially in light of the magnitude of the effects estimated from our sample. The widespread use of incarceration has the capacity to cause marital instability from which offenders are unable to recover in the 5-year period following their first incarceration experience. When we investigated whether subsequent convictions or incarceration spells confounded our estimates of the impact of incarceration on divorce, we discovered that they did not. The results were thus quite robust. The precise mechanism for the incarceration effect on divorce is unclear, however, and unfortunately cannot be parsed out using our data. We can suggest at least three interrelated possibilities, which point to processes that are external (restrictions imposed by society), internal (developmental changes within individuals), and transactional (unresolved strains between spouses). We consider these to be important avenues for continued research on the incarceration–divorce relationship.

The first potential mechanism is *social exclusion*, linked to the stigma of a prison record. For example, incarcerated offenders might face difficulty finding employment and therefore helping to provide financial support for their families (Pager 2003; Western 2002). They might also be treated as outcasts by others in their social network (Becker 1963; Braithwaite 1989), which we point out would negatively impact not only the

[22] This is not a universal sentiment, however. There remains some evidence that imprisonment can have a substantial deterrent effect on criminal behavior (e.g., Bhati and Piquero 2008).

[23] The underlying causal mechanism is not necessarily attributable to the signal that such confinement sends about an offender's risk for domestic violence, however, since the divorce likelihood does not vary by offense severity among convicted men. It is quite possible that this is an artifact of sentence length. Simply put, serious offenders serve longer terms of confinement and thus face lengthier time out from marriage, which can strain marital bonds.

incarcerated offender but his family as well. The marriage might therefore dissolve as a result of the strain of one's status as an "ex-offender," due to the inability to completely conceal one's criminal past from others.

The second possible mechanism is *criminal socialization*, linked to the prison experience itself. Exposure to prison might harden inmates and create problems of adjustment to the outside world (Clemmer 1940), including the family. Or incarcerated offenders might adopt a criminal self-concept and the repertoire that such a label presupposes (Lemert 1972; Schur 1971), an identity that is not necessarily friendly to strong "family values." These mechanisms imply that, through the actual experience of incarceration and exposure to other inmates, offenders become increasingly embedded in crime and a criminal lifestyle (Hagan 1993). The likelihood of divorce thereby increases because offenders experience difficulty adapting to life as a conventional, law-abiding citizen.

The third intervening mechanism is *marital disruption*, linked to the forced separation between spouses during the period of the offender's confinement. Western (2006) observes that imprisonment has a "corrosive effect on family life" (p. 133), not least because individuals must overcome a number of obstacles to maintain close communication with their incarcerated spouses (see also Lopoo and Western 2005). The "surviving spouse" might become more independent as a result of the loss of communication and emotional support. Divorce therefore becomes more likely since the strain of separation erodes the marital bonds that existed between spouses prior to the offender's confinement.

As one final discussion point, at the request of one of the co-Editors, we also estimated the models for 10-year outcomes in order to ascertain if the results observed herein persist past the 5-year mark. They do indeed. The 10-year marriage probability associated with incarceration in the nearest neighbor matching model is neither statistically nor practically significant, and in fact is positive (ATE = .0044; ES = +.01). The 10-year divorce probability, on the other hand, was positive, statistically significant, and substantively meaningful (ATE = .1761; ES = +.42). Therefore, we observe erosion in marriages attributable to incarceration that continues to grow over time, for up to 10 years following first-time imprisonment.

Conclusion

This study adds to the growing research on the detrimental impact of incarceration on life outcomes, and has implications for life-course/developmental criminology and empirical tests thereof. The findings indicate that first-time imprisonment among Dutch male offenders has no causal impact on the likelihood of marriage, other than a residual incapacitation effect in the first year post-confinement. Beyond 1 year, however, any effect of imprisonment on marriage disappears, at least relative to a sample of convicted individuals who were at high risk of imprisonment. In the case of transitions to marriage, therefore, incarceration does not appear to be a salient turning point in the life course of offenders, at least when incarcerated offenders are properly compared to similarly high-risk men who were not (yet) incarcerated. Put simply, incarceration does not appear to erode an offender's "marriageability," at least to no additional degree beyond the influence of characteristics that determine his risk of conviction.

The same cannot be said of transitions to divorce, however. There is strong evidence that incarceration has a causal impact on the likelihood of divorce. This effect is substantively large and grows with the passage of time, persisting for up to 10 years following first-time imprisonment. In short, among convicted offenders who are already married, first-time incarceration represents a substantial, negative turning point in one's marriage trajectory. We believe that life-course criminology would greatly benefit by explicating and testing the mechanism(s) by which incarceration undermines marital stability.

We should qualify our conclusions by drawing attention to unmarried cohabitation as a growing living arrangement in the United States and other Western, industrialized societies (Casper and Bianchi 2002; Kiernan 1999). Cohabitation is so normative that it has even become a substitute for marriage, and The Netherlands is no exception to this trend. This is less of a problem for our analysis because a large share of the CCLS men had reached marriageable age prior to 1970, when cohabitation was far less prevalent. Yet we regard this topic as an essential avenue for continued exploration, as Lopoo and Western (2005) and Western (2006) have done.

What is noteworthy, we believe, about the estimates reported herein is that first-time incarceration in The Netherlands is only three-and-a-half months in length, on average. This is akin to jail incarceration in the United States. That short-term confinement is associated with long-term (up to 5 years) marital instability is not encouraging in light of the expanded use of custodial sanctions in response to crime in The Netherlands and elsewhere. To be sure, we cannot yet rule out the possibility that underlying differences between The Netherlands and the United States explain the findings reported here. We thus believe that confirmation of our results from existing US datasets, using similar methods, should be a research priority.

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Appendix A

Details on the Estimation of Rosenbaum Bounds for the Average Treatment Effect of Incarceration on Marriage Formation and Stability

In this appendix, we elaborate on the logic and estimation of "Rosenbaum bounds" for the sensitivity of the average treatment effect (ATE) to hidden bias. Our description draws from Rosenbaum (2002), Becker and Caliendo (2007), and DiPrete and Gangl (2004). Our application involves estimation of the impact of incarceration on the likelihood of marriage and divorce. The sensitivity analysis begins with the formulation of the usual propensity score, but supplements the model with an additional, unobserved component:

$$\pi = \Pr(\text{Inc} = 1) = \frac{\exp(\alpha + \beta X + \gamma U)}{1 + \exp(\alpha + \beta X + \gamma U)}$$

where X are the observed variables, U is an unobserved variable, and α, β, and γ are unknown parameters. Because our model conditions only on individuals who were convicted of a crime, we may conceive of U as some kind of "crime risk trait" that is observed by a sentencing judge but is unobserved by the researcher, and whose presence increases the probability of receiving a sentence of incarceration by a factor of γ. The familiar logit (log-odds) formulation of this model is represented as:

$$\ln\left(\frac{\pi}{1-\pi}\right) = \ln\left[\frac{\Pr(\text{Inc}=1)}{1-\Pr(\text{Inc}=1)}\right] = \alpha + \beta X + \gamma U$$

and exponentiation leaves us with the odds of incarceration for an arbitrary individual in the sample:

$$\frac{\pi}{1-\pi} = \frac{\Pr(\text{Inc}=1)}{1-\Pr(\text{Inc}=1)} = \exp(\alpha + \beta X + \gamma U)$$

If there is hidden bias or unobserved confounding, two paired individuals i and j with the same observed covariates X (or the same propensity score) still have different chances of being incarcerated. The problem can be illustrated by taking the ratio of the odds of treatment for these two individuals:

$$\frac{\pi_i/(1-\pi_i)}{\pi_j/(1-\pi_j)} = \frac{\exp(\alpha + \beta X_i + \gamma U_i)}{\exp(\alpha + \beta X_j + \gamma U_j)}$$

where i indexes an incarcerated (treated) individual and j indexes a non-incarcerated (untreated) individual. Because the propensity score balances X, this odds ratio (or odds multiplier) can be algebraically simplified:

$$\frac{\exp(\gamma U_i)}{\exp(\gamma U_j)} = \exp\left[\gamma\left(U_i - U_j\right)\right]$$

A study is free of unobserved confounding only if there are no differences in unobservables (i.e., $U_i = U_j$ for all matched pairs i,j), or the unobservables do not influence the probability of incarceration (i.e., $\gamma = 0$). In the absence of direct information on the unobservables, however, Rosenbaum (2002) proposes a simulation that subjects γ to perturbations as a way to assess its influence on treatment effect estimates. In order to make the sensitivity analysis tractable, we can impose the simplifying assumption that U is a dummy variable, that is, it is either present or absent. This implies the following bounds on the odds ratio that either of two individuals matched on X will be incarcerated:

$$\frac{1}{\Gamma} \leq \frac{\pi_i/(1-\pi_i)}{\pi_j/(1-\pi_j)} \leq \Gamma$$

where

$$\Gamma = \exp(\gamma)$$

A number of sensitivity statistics are available for matching estimators depending on the distribution of the response variable and the matching procedure used (for review, see Rosenbaum 2002). With a binary outcome and one nearest neighbor, we employ Mantel and Haenszel (1959) test statistic. This is a non-parametric test that compares the observed number of incarcerated (treated) men that are married (or divorced) to the expected number given that the treatment effect is zero. We adapt the notation of Becker and Caliendo (2007) to our particular application:

$$Q_{\text{MH}} = \frac{\left|Y_1 - \sum_{s=1}^{S} E(Y_{1s})\right| - 0.5}{\sqrt{\sum_{s=1}^{S} \text{Var}(Y_{1s})}} = \frac{\left|Y_1 - \sum\limits_{s=1}^{S}\left(\frac{N_{1s}Y_s}{N_s}\right)\right| - 0.5}{\sqrt{\sum\limits_{s=1}^{S}\left(\frac{N_{1s}N_{0s}Y_s(N_s-Y_s)}{N_s^2(N_s-1)}\right)}}$$

where s represents a single stratum ($s = 1,2,\ldots,S$) or, in our case, a single matched set and

Y_1 = the total number of imprisoned or treated individuals in the sample who are married (divorced).
Y_{1s} = the total number of imprisoned individuals in matched set s who are married (divorced).
Y_s = the total number of individuals in matched set s who are married (divorced).
N_{1s} = the number of imprisoned individuals in matched set s (=1 in our study).
N_{0s} = the number of non-imprisoned individuals in matched set s (=1 in our study).
N_s = the number of individuals in matched set s.

For fixed $\Gamma \geq 1.0$ and $U \in \{0,1\}$, results shown in Rosenbaum (2002) demonstrate that Q_{MH} can be bounded by two known distributions, providing an upper and lower bound. Using notation from Becker and Caliendo (2007), the upper and lower bounds, respectively, are given by:

$$Q^{+}_{\mathrm{MH}} = \frac{\left|Y_1 - \sum_{s=1}^{S} \tilde{E}^{+}_{s}\right| - 0.5}{\sqrt{\sum_{s=1}^{S} \mathrm{Var}\left(\tilde{E}^{+}_{s}\right)}}$$

and

$$Q^{-}_{\mathrm{MH}} = \frac{\left|Y_1 - \sum_{s=1}^{S} \tilde{E}^{-}_{s}\right| - 0.5}{\sqrt{\sum_{s=1}^{S} \mathrm{Var}\left(\tilde{E}^{-}_{s}\right)}}$$

with details on the large-sample approximations of $\tilde{E}^{+}_{s}$ and $\mathrm{Var}\left(\tilde{E}^{+}_{s}\right)$, as well as $\tilde{E}^{-}_{s}$ and $\mathrm{Var}\left(\tilde{E}^{-}_{s}\right)$, given in Becker and Caliendo (2007, p. 74, note 5).

Appendix B

See Table 5.

Table 5 Balance diagnostics for imprisoned and non-imprisoned offenders, full and matched samples

	Full sample				Matched sample			
	Unmarried		Married		Unmarried		Married	
	t-stat.	ES	*t*-stat.	ES	*t*-stat.	ES	*t*-stat.	ES
Offense characteristics								
Category of offense								
Simple theft	−4.92	−0.18	−1.39	−0.10	0.02	0.00	0.20	0.02
Rape	6.04	0.18	6.35	0.31	−0.02	−0.00	0.34	0.03
Sexual assault	2.19	0.07	2.46	0.15	−1.19	−0.06	−0.34	−0.04
Sexual abuses/other sexual offense	1.76	0.06	−1.72	−0.13	0.68	0.03	−1.49	−0.20
Threatening	2.77	0.09	0.69	0.05	0.81	0.04	−0.76	−0.07
(Attempted) murder/manslaughter	1.93	0.06	2.40	0.13	−0.36	−0.02	0.53	0.06
Assault	−0.36	−0.01	0.31	0.02	−0.47	−0.02	0.60	0.06
Violent theft	8.88	0.26	4.79	0.24	0.53	0.02	−0.11	−0.01

Table 5 continued

	Full sample				Matched sample			
	Unmarried		Married		Unmarried		Married	
	t-stat.	ES	*t*-stat.	ES	*t*-stat.	ES	*t*-stat.	ES
Extortion	4.43	0.13	2.74	0.15	1.89	0.08	1.00	0.05
Forgery	−1.50	−0.05	−0.18	−0.01	−0.54	−0.02	1.64	0.18
Aggravated assault	7.04	0.24	4.80	0.30	−0.58	−0.03	−0.55	−0.06
Embezzlement	−1.59	−0.06	0.20	0.01	−0.31	−0.01	0.04	0.01
Swindling	−0.44	−0.02	−0.73	−0.05	−1.11	−0.05	−0.58	−0.08
Fencing	−2.42	−0.09	−0.76	−0.05	−0.23	−0.01	0.42	0.05
Public order	−2.33	−0.08	−1.24	−0.09	1.41	0.05	−0.82	−0.08
General offending	1.12	0.04	1.63	0.09	1.50	0.07	–	–
Offense against authority	−3.16	−0.12	−2.47	−0.19	0.53	0.02	0.00	0.00
Flashing	−5.05	−0.20	−5.19	−0.42	0.91	0.02	0.00	0.00
Vandalism	−8.37	−0.33	−2.66	−0.20	0.12	0.00	0.58	0.05
Other criminal law	−2.17	−0.08	−2.36	−0.20	1.46	0.04	−0.81	−0.04
Opium act	−0.76	−0.03	1.20	0.08	−0.49	−0.02	0.28	0.03
Weapons act	−0.25	−0.01	−0.60	−0.04	−0.16	−0.01	−0.67	−0.09
Severity of offense	7.02	0.24	3.56	0.22	0.55	0.03	−0.13	−0.01
No. crimes in case of conviction								
Violent crimes	4.38	0.14	4.09	0.21	−0.19	−0.01	1.22	0.09
Property crimes	13.02	0.39	6.74	0.34	−0.88	−0.00	0.51	0.04
Other crimes	4.20	0.14	3.99	0.23	−0.17	−0.02	1.05	0.11
Criminal history								
No. crimes in past 5 years								
Violent crimes	0.42	0.01	1.20	0.08	−1.55	−0.07	0.28	0.03
Property crimes	9.16	0.30	3.57	0.23	−0.12	−0.01	−0.09	−0.01
Other crimes	0.67	0.02	0.33	0.02	−1.80	−0.09	1.17	0.13
No. crimes beyond past 5 years								
Violent crimes	−3.79	−0.14	−1.69	−0.12	−0.65	−0.02	0.15	0.01
Property crimes	−3.69	−0.13	0.02	0.00	−0.36	−0.01	0.55	0.06
Other crimes	−3.16	−0.11	−2.40	−0.18	0.51	0.02	0.21	0.02
Early conviction (before age 16)	3.39	0.12	−0.08	−0.01	−0.72	−0.04	−1.18	−0.12
Life circumstances								
Current marital and fertility status								
Not married—no children	1.13	0.04	–	–	−0.17	−0.01	–	–
Not married—children	−0.27	−0.01	–	–	−0.72	−0.03	–	–
Married—no children	–	–	−2.42	−0.17	–	–	0.30	0.03
Married—children	–	–	2.42	0.17	–	–	−0.30	−0.03
Separated—no children	−1.45	−0.05	–	–	0.39	0.01	–	–
Separated—children	−0.54	−0.02	–	–	0.77	0.03	–	–
Marital and fertility history								
No. years married in past 5 years	−0.13	−0.00	−1.90	−0.13	1.06	0.04	−0.08	−0.01

Table 5 continued

	Full sample				Matched sample			
	Unmarried		Married		Unmarried		Married	
	t-stat.	ES	*t*-stat.	ES	*t*-stat.	ES	*t*-stat.	ES
No. years have children in past 5 years	−1.01	−0.04	0.02	0.00	−0.06	−0.00	0.47	0.05
Sociodemographics								
Non-Dutch	6.39	0.21	4.34	0.27	0.94	0.04	−0.16	−0.02
Birth cohort								
Cohort 1	−6.17	−0.21	−1.60	−0.11	0.05	0.00	0.73	0.07
Cohort 2	1.61	0.06	0.11	0.01	−0.24	−0.01	−0.16	−0.02
Cohort 3	6.26	0.21	1.15	0.08	0.23	0.01	−0.34	−0.04
# Covariates imbalanced (%)	25 (58%)	10 (23%)	19 (46%)	12 (29%)	0 (0%)	0 (0%)	0 (0%)	1 (3%)

A *t*-statistic that is |±1.96| or larger is considered imbalanced, as is an effect size (ES) or standardized difference that is |±0.20| or larger

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